GÖTEBORGS UNIVERSITET

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Will more funding generate better academic results in primary school?

- A quasi-experimental study of the centralization reform in Stockholm 2007

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Abstract

The fact that education plays a major part for wealth of nations has been known for quite some time, but the debate over the true relationship between school funding and student performance is ongoing in both academia and among policymakers. They all want to answer the same question - how can the returns of education be maximized?

We use a centralization reform in Stockholm from 2007 to estimate the causal effect of increased school funding on student performance. We study three measurements: Grade point average, the amount of students that at least got a pass in every class and the amount of students that got the grades required to graduate. The studied schools are public primary schools in Stockholm.

The study was done by using a difference-in-difference approach with GPA, Pass and Grad as our outcome variables. Our findings show that the reform created clear winners and losers in terms of school funding and that the majority of school budgets were unchanged. Our results however show no statistically significant short term effects on student performance whether a school was a winner or not.

Keywords School funding, Student performance, School centralization reform

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Contents

List of Figures

List of Tables

1 Introduction

In 2007 the newly elected center-right coalition in Stockholm decided to centralize the responsibility for public primary schools from 18 districts to one central School and Education Division. This reform affected the schools in two ways: 1) A new management system and 2) a new resource allocation model. The new organization had all principals answering directly to a central education division in contrast to the system before in which all the districts[1] had their own school division. The change in school funding was a consequence of the new organization. The districts could no longer influence how much money a school would recieve. The new model treated every school equal and it made some schools winners and some losers in terms of funding.

Even though this restructure created both winners and losers we will not study the effect of decreased school funding; this is partly because almost no schools were losers in absolute terms since the policymakers increased the total funding during the years after the reform in order to make the implementation more accepted. We also wanted to limit our research question to that of increased funding instead of studying change in resource funding, this due to studies claiming that the effect is different for an increase and a decrease in funding (Heller Sahlgren 2014).

There are two things that we are able to say with this quasi-experimental study. The first is that there were clear winners and losers after the reform. The winners were mainly schools with a student body with lower socioeconomic status, the opposite being true for losers. The second conclusion that can be drawn is that our results indicate, but do not prove that there is a positive effect on student performance of increasing school funding. We cannot show a statistically significant relationship between resources and our student performance variables. This could either be because the data and methods available for this thesis are not sufficient. It could also be because there is no causal effect.

This thesis consists of nine sections. Section 1-5 describes the centralization reform in detail and how we define winners and losers. Section 6-7 examine if the winners in terms of money also became winners when it comes to student performance. In section 8-9 we discuss the results and present the conclusions. There are three appendices that cover the school organization in Stockholm, data and results.

2 Literature review

The literature on resources effect on students' performance is vast to say the least. But even though the number of studies is impressive the literature lacks an overarching consensus. One of the most cited papers is Hanusheks meta-analysis of 377 studies, which concludes that there is

[1] See appendix A for more description on the school organization in Stockholm before the reform

no discernible effect on student performance caused by an increase in spending (Hanushek 1997). Other quantitative reviews on the contrary argue that resources have a positive effect (Hedges et al. 1994). One of the most well-known studies that shows a positive effect is the STAR-project which also serves as a baseline for many studies when comparing effect size. We also want to mentioned that there are studies showing that students and teachers react differently to an increase compared to a decrease in resources (Heller Sahlgren 2014). This, and also delimitation, servers as reasons for why this thesis only will study an increase in school funding.

Hanusheks review is arguably the most cited work in this field of research but it has more recently been exposed to critique. Lindahl and Kreuger show that the conclusions drawn from Hanusheks meta-analysis is highly dependent on how the reports are weighted in the meta-analysis (Krueger and Lindahl 2002). Most of the reports Hanushek bases his analysis on were also made before the 90's and since then new methods and data has become available. A few less cited but more recent studies have strengthened the evidence for a positive relation between resources and student performance. They also emphasize that experimental or quasi-experimental studies are superior to studies relying on observational data. One recent working paper uses a research design that includes court rulings in USA as a quasi-experimental identification strategy Jackson et al. 2014. They find that resources do matter for students from poor families, while they do not find any significant effect on students from non-poor families. They also stress the importance of using exogenous changes to estimate the effect of changes in school funding.

> "The stark contrast between the OLS and the 2SLS estimates underscores the importance of relying on exogenous variation in school spending. Importantly, the contrast between the OLS and the 2SLS estimates in our data provides an explanation for why these estimates might differ from other influential studies in the literature (e.g., Coleman et al., 1966, Betts, 1995, Hanushek, 1996, and Grogger, 1996). We suspect some prior studies that lacked a compelling research design to isolate causal effects of spending may have produced modest estimated effects of school spending due to unresolved endogeneity biases." (Jackson et al. 2014)

This is also commented on by Fredriksson and Öckert 2007 who made the same observation. In their study they use the decentralization policy from the early 90's in Sweden as a natural experiment. They get significant results with roughly the same effect size as the STAR-project. An even more recent study use the fact that Sweden earlier had a cap on class sizes to analyze the long term effect of class size (Fredriksson, Öckert, and Oosterbeek 2012). They further strengthen the notion that there are interesting results to be found within the field of quasi-experimental studies on school funding. Fredriksson and Öckert also claim that there are very few studies done on European data with a credible identification strategy.

Even though there are some tendencies in the current research that could change the old consensus, most researchers seem to agree that increasing school spending alone is not an efficient way to increase student performance (Hanushek 1997). One of the problems is that even if we know that a school got more resources, we might not necessarily know that they used it efficiently and how or on what they spent it on (Hanushek 2003). This makes it hard to make any inference and the better studies usually have richer data sets that make it possible to control for these factors. These kinds of data sets are not all too common, since they require a lot of time and resources to construct.

One final topic that we feel the need to mention is the theory that of schools and their functional form. We will not go into this in any great detail for delimitation reasons, though we felt that this was too important to leave out since it is could be one of the reasons that we do not find any significant effect in this study. Figlo (1999) argues in his article *Functional form and the estimated effects of school resources* that one of the reasons to why studies have failed to show a strong correlation between resources and student performance is due to making the wrong assumptions about schools and their functional form. Figlo argues that one cannot simply compare an impoverished school with a rich school, since they most likely does not share the same functional form (Figlo 1999). This will be covered in more detail in the discussion.

3 Institutional background

3.1 Background to the reform

The reform was a major change to the school organization in Stockholm. Since early 1990's the public primary schools had been governed by 18 separate district boards within the city. Even though taxes were collected at the municipality level and then distributed to the district, every district could up to a point make their own decisions about their schools' budgets. The degree of autonomy for the districts had been a highly debated political issue around which the opposing political alternatives were fighting. Between 1998-2002 when a center-right coalition governed the city and the Liberal Party was in charge of school policy they earmarked some of the district funding for education. After this at least 70 percent of the funding that was intended for education had to be allocated directly to the schools. When a left wing majority regained power in 2002 they left the base funding largely unchanged but added a second school grant. This grant was substantially smaller but based on a socioeconomic index in order to create a more equitable school funding. It was also a way to produce more transparency and predictability since it replaced a more complex system of compensating schools for various cost driving students. The money however was not allocated directly to schools but rather to the districts and could thus be distributed in any way

they found fit, they did not even have to use them for school purposes.

3.2 A centralized school organization

When the center-right majority reemerged as winners in the election of 2006 they decided to centralize the whole school organization. The reform was implemented over the summer of 2007 and after that every school faced the same resource model that was decided by the city council, not the districts. The new model had roughly the same parameters as the old model the city had used to allocate money to the districts. The biggest difference was that the schools, and not the districts, now got full authority to spend the money. The parameters in the model included the number of students in different ages and the socioeconomic composition of the schools. This is the reform that we are studying in this thesis.

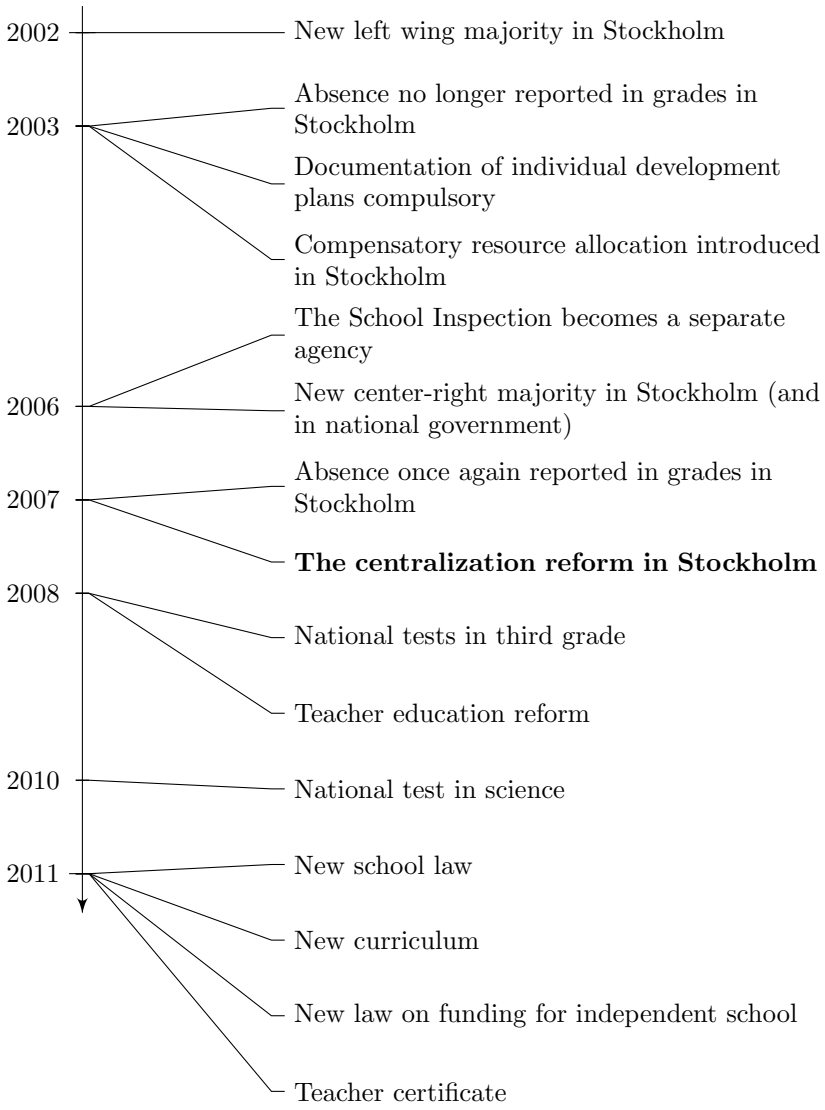
The reform can be characterized as an exogenous shock for the schools. The official rational behind the reform was to create a transparent, fair and equal system where every school faced the same resource allocation model without loopholes. Here is a quote from Lotta Edholm, deputy major and responsible for the restructure taken from a newspaper at the time of the implementation:

> "- Ett enkelt, genomskinligt och mer rättvist system. Tidigare hade vi arton olika sätt att sköta våra skolor, nu har vi ett. Och jag kan garantera att rektorerna kommer att bli nöjda, säger hon."[2] (By 2007)

Centralization was also a part of the political agenda that the Liberal Party was promoting. They wanted to centralize the school organization in Sweden as much as possible. This would indicate that the reform was motivated by political reasons and was a consequence of the new political majority. The interviews we have conducted with Lotta Edholm, representing the political majority at the time and Johanna Engman; a public official with responsibility for implementing the reform, tell two different stories about the immediate rational for the reform. Mrs Edholm emphasized the alleged mismanagement of some districts and that money intended for education was used elsewhere. Mrs Engman said on the contrary that almost all money the districts got for education was used for that purpose. There are two sides to this story but there were undoubtedly multiple reasons for the reform. We can however be confident that there was no direct correlation between student performance and the change in resources imposed by the reform. Since the school themselves were unable to affect the outcome this is another indication that this was an exogenous shock. This does not however allow us to assume that there are no endogeneity bias what so ever when analyzing the data. For that we would need an actual experimental setting.

[2] "- A simple, transparent and more equitable system. Previously we had eighteen different ways of running our schools, now we have one. And I can guarantee that the principals will be satisfied, she says."

Figure i: Timeline of school reforms



This is not an exhausting list of all reforms during this period but rather a selection of the most relevant for this thesis.

3.3 Other school reforms around this time

This was the major education reform in Stockholm around this time and the resource model had not been subject to any larger changes since the last reform in 2003. Around 2011-2012 the model was changed again (Burestam 2010) along with the national legislation for independent schools that changed the resource allocation from municipalities (prop 2009/10:157). Even though no other major changes to the resource allocation were made during the 2-3 years around the centralization reform there were a few other school reforms going on within this general time frame. Figure i outlines some of the bigger events from 2002 to 2011.

The policy changes that happened around the same time as the centralization reform could cause a problem when analyzing the causal effect of the reform. Fortunately none of these other

reforms seem to have a clear effect on student performance.

Reporting absenteeism in the students' grades was reintroduced around the same time as the centralization but there is not much evidence to support that it had any effect on student performance. Absenteeism was of course reported even before 2007, the difference being only whether they were written in the grades or not. In addition to that the absenteeism was never reported in the final grade that students use to apply for high school or to get a job.

The introduction of national tests in third grade was a big reform which demanded that all students should take the same tests in Swedish, Mathematics and English. However it did not impact the students we observe in this paper. The same thing goes for the teacher education reform.

In 2010 all ninth graders had to take a mandatory test in science (Lundqvist and Lidar 2013), this would directly impact the students we are studying. It is likely that their grades in science dropped as a consequence of this. We base this assumption on the fact that national tests impact how teachers grade their students (Skolverket 2009).

The earlier reform to the resource allocation model in 2003 will most probably impact the schools and we want to make sure that our observed results are not intertwined with the effect of that reform. That is why our preferred time window to study this reform is 2006-2009. Both the national test in science and of the huge amount of reforms that was implemented in 2011 will be our reasons for not including 2010.

4 Data

The sample includes all public primary schools in Stockholm with graduating students two years before and after the reform.[3] That include 59 schools and 5 of those were excluded from our data-set for reasons mentioned below.

There are four kinds of data in our thesis; 1) data on school funding, 2) data on student performance, 3) data on student background and 4) data on school size. The student performance, background and school size data comes from the two national databases SALSA (Skolverkets Arbetsverktyg för Lokala SambandsAnalyser) and SIRIS (Skolverkets Internetbaserade Resultat- och kvalitetsInformationsSystem) and the data on school funding was collected from the School and Education Division and the District Councils in the City of Stockholm (Stockholm stad).

It is important to note that the years in our data and in this study in general refer to the academic years and not calendar years. When an arbitrary year is referred to in this study, for instance 2007[4], it means the academic year 2006/07. In these terms the centralization reform took

[3] See table B1 in appendix B for a list of all schools.

[4] When referring to half years/semesters the notation 2005:1 or 2005:2 is used to indicate if it is the first or second half year.

place between 2007 and 2008 but in terms of actual dates the reform was implemented on the first of July 2007.

4.1 Measuring school funding

We are studying a reform of the resource allocation model i.e. budgets, but budgets do not always reflect how much money a school actually spent. So we decided to look at the accounting from every school and from that derive the total cost per year. Schools manage their economy by the calendar year and not by the academic year, so in order to match a schools cost for a full academic year we collected cost data by half years. For the post-reform period that data are readily available but the pre-reform data is not perfect in this respect. In 13 of the 54 schools we had to take the full year cost and divide by two in order to get an approximate half year cost. This is not ideal, especially because the spring semester is a few days longer than the fall semester. But this should not be a major problem since the undoubtedly largest cost for all schools are teacher salaries and they are approximately the same for each semester.

The data is collected on individual school level and from 15 different data sources[5]. The pre-reform data was gathered from the district council that was responsible for the schools at that time. The data from the post-reform period was gathered from the School and Education Division. All economic accounting in the City of Stockholm both before and after the reform had some common routines, but not on every aspect. We do not know the details of these exemptions and which districts had them, but the assumption is that the majority of the accounting followed the same structure before and after the reform. But since we can not be sure we might get some measurement error from this that will bias our results towards zero.

One thing we know is that there was a difference between different districts whether schools paid for rent, electricity and the salary of the principal before the reform. After the reform this was the same for all schools. In order to control for this variation, all rents are excluded from the cost-data[6]. Electricity and principal wages are harder to control for and we cannot be sure that they do not bias our treatment variable. The average wage for a principal at a public primary school in Stockholm County in 2013 was approximately 850 000 SEK per year according to The Swedish Association of School Principals and Directors of Education (Skolledarförbund 2014). The total cost for a school during the period that we are studying ranges from 23 million SEK up to 95.5 million SEK with an average on 53 million SEK. This would imply that principal salaries would adhere to on average 1.5% of the schools' costs and as much as 3.6% for the smallest schools. The

[5]One is the School and Education Division and the 14 others are the districts, they are 14 and not 18 since 4 districts has been reorganized since the time of the reform

[6]In the code of accounts all rent costs have the code 510 both before and after the reform, thus allowing us to be certain that the correct amount was excluded. For some district there were other code of account, but all divergence from this general rule are displayed in table B2 in appendix B

cost for electricity after the reform, when all schools paied for it, was on average lower than 1% of the total cost. The conclusion we have drawn from this is that even if we do have some measurement errors, these are likely to be small and will probably not bias our results in a significant way since we are measuring the effect of increasing school funding by at least 20 percent.

With the cost data we constructed a cost per student variable. One threat to the validity of this variable is that some schools have grade F-9[7] and some have only grade 7-9. This is a concern since we know that the resource allocation was different for students in different grades[8]. Grade composition does not however change a lot from year to year.

There are likely other factors that inflate our cost per student-variable. One is the costs for students with intellectual disabilities and other special needs. The share of these students varies quite a lot between schools and they demand a lot more resources then other students. The schools are compensated for this but we cannot control for it when creating the cost per student variable. Even though we cannot disentangle their cost, we have data on the number of students with intellectual disabilities and we can see that their share of every school stays relative constant during this period.

The implication of this is that a comparison in cost per student between schools is difficult when we do not have individual data. There are reasons to believe that there are a number of factors that inflate or deflate the value of some schools. When we look at our data see that these factors seem to stay constant over time so it will not decrease the reliability just the validity of our observed values. However if we compare relative changes in cost per student instead of absolute changes we should not have the same problem.

Furthermore we had some schools showing up extreme values and they were removed from our data set. This is not based on the assumption that outliers should be removed, but the fact that they had reported their spending in a very different way than the other schools. In some cases there are confirmed accounting errors that made us exclude schools. There is a detailed description of the schools we removed and why in table B3 in appendix B.

4.2 Student data

All the data on student performance, background and school size have been collected at the school level and not from individual data. The three standard measurements on student performance used in the primary schools in Sweden are Grade point average (GPA), graduation rate (Grad) and the percentage of students that get a passing grade in all subjects (Pass). The most conventional way to measure student performance is however GPA and that will be our preferred measurement. A students GPA was at this time calculated as the sum of 16 grades that can take one of the

[7] F stands for preschool class (förskoleklass) which is a voluntary education form for 6-year-olds
[8] More precise: there was one sum for students in grade 1-3, another for grade 4-6 and a third for grade 7-9

following values: 0, 10, 15, 20 (a passing grade renders 10 points). The pass-measurement is rather straight forward but it is worth noticing that a change only reflects changes between passing and not passing grades. The graduation rate is also easy to interpret, in order to graduate from primary school these years you had to get a passing grade in at least Mathematics, Swedish and English. An increase in the graduation rate would imply that the worst performing students increased their grades.

Table 1: Variable list

Variable	**Discriptions**
GPA	Mean grade point average
Pass	Percent of students who get a passing grade in all classes
Grad	Percent of students who get a passing grade in Math, Swedish and English
Cost per student	Total cost for a school divided by the number of students 1-9
Cost inf per stud	Inflation adjusted cost per student
Cost inf res per stud	Inflation and time trend adjusted cost per student
Log cost	Log of the inflation adjusted cost per student
Born abroad	Percent of students who are born abroad
Parent abroad	Percent of students with one or both parent born abroad
Parent educ	The average educational attainments of both biological parents where 1 indicate primary, 2 secondary and 3 tertiary education as their highest level.
Proc boys	Percent of student that are boys
Winners	A dummy indicating if the school is a winner after the reform (Treatment group)
Losers	A dummy indicating if the school is a loser after the reform
Unchanged	Unchanged after the reform
Non-winners	Unchanged and losers of the reform (Control group)
Treatment	Percent increase in inflation and time trend adjusted cost per student after the reform
Post	A dummy that is 1 if year = or > 2008
Year	A vector of dummies indicating academic year
Time	A time series that is 1 if year=2006, 2 if year=2007 ...
Time2	The squared value of time
Winners x post	Interaction between winners and post
Winners x 2008	Interaction between winners and year dummy for 2008
Winners x 2009	Interaction between winners and year dummy for 2009
District	Indicated what district a school belonged to pre-reform[a]
School ID	A unique identifier for each school
Alt-ID	When School-ID has changed over time an alternative is presented

[a]In appendix A all the districts and their corresponding dummy is presented

Table 2: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
GPA	216	219	28.6	144	276
Pass	215	74.2	16.1	28.3	98.4
Grad	216	88.4	12.0	41.3	100
Cost per stud	216	91328	27885	44616	184969
Cost inf per stud	216	87799	26286	44305	172878
Cost inf res per stud	216	80825	25915	41608	161254
Log cost	216	11.3	0.29	10.7	12.1
Parent edu	212	2.25	0.30	1.44	2.86
Proc boys	212	51.6	8.08	25.0	76.0
Parent abroad	212	17.6	16.4	0.00	77.0
Born abroad	212	14.0	14.5	0.00	71.0
Winners	216	0.15	0.36	0	1
Losers	216	0.13	0.34	0	1
Unchanged	216	.7	.4	0	1
Non-winners	216	.9	.4	0	1
Treatment	216	.7	17.1	-36.9	39.1
Post	216	0.50	0.50	0	1
Time	216	2.50	1.12	1	4
Time2	216	7.50	5.69	1	16
Winners x post	216	0.07	0.26	0	1
Winners x 2008	216	0	.2	0	1
Winners x 2009	216	0	.2	0	1

Note: There is missing values from one school on the socioeconomic composition and there is one school without a value on Pass for 2009.

5 Defining winners and losers

The reform created winners and losers in terms of funding. But what is a credible way to categorize the schools? How much more money must a school receive in order to be classified as a winner? The creation of such categories is by default an arbitrary process, but there are some methods that make more sense than others.

We start out by creating an inflation and time trend adjusted value for cost per student for every school. The inflation rate comes from SCB 2015 and is presented in table 3. The inflation adjusted costs are then used in the following regression:

$$\text{Half year inflation adjusted cost per student}_{it} = \alpha_i + \beta_1 \tau_t + \epsilon_{it} \quad (1)$$

Where τ is a time trend for each of the half years we have in our data (with $\tau = 1, 2, 3 \ldots 11$).

From this regression we get that $\widehat{\beta_1} = 774.8684$ we use this value to time trend adjust the inflation adjusted costs. This intermediary step is not absolut necessary but will make the process more intuitive. The trend can be seen when comparing figure iii with figure iv, looking at the unadjusted graph we can see that we have a clear upward trend, which is gone in the adjusted.

We compare relative changes rather than absolute increases and decreases in cost per student since the min- and max-values for cost per student are quite far spread out. A 5 000 SEK per student increase for one school might be seen as a substantial increase, while at the same time being considered a small increase for another. So the winners and losers will be defined by looking at their relative change pre- and post-reform.

Table 3: Inflation

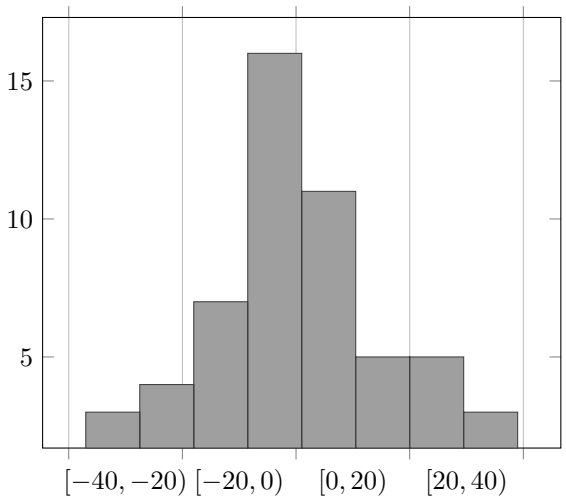
Year	Index
2005	100.00
2006	101.40
2007	103.63
2008	107.15
2009	106.83
2010	108.22

In order to cancel out noise we compare the average inflation and time trend adjusted cost per student before and after the reform and then use the percent change as our preferred variables on how much a school was affected by the reform. For this we use the cost data from the calendar year 2005:1-2010:1. The reason for using more years when defining winners and losers and not using those years for our regression is that the resource trend is stable over the entire period with the reform year as an exception, which is not the case for GPA, Grad and Pass for the reasons mentioned in section 3.

This process will give us a variable that we call *treatment*. Figure ii shows a histogram over the distribution of the treatment. It varies from a decrease of 40 percent to roughly an increase in 40 percent.

From this variable we apply a symmetric high and low cut-off point that decides if a school is considered a winner or loser. In order to come up with a reasonable cut-off value we considered how much an ordinary school usually deviate from its average value without the reform year taken into account. Our data shows that it deviates up to about 10% and with an average deviation of 5%. We then tried different cut-off points in order to see if we received the same results for different cut-off points and the results came out similar for 15%, 20% and 25% (see appendix B). The 20% cut-off point will however be the preferred since the 15% cut-off came a bit too close to what could have been an ordinary fluctuation and the trade

Figure ii: Histogram on treatment



of just being that we got a 10% smaller winner group. The 25% cut-off would be better, since the effect is greater, though choosing this would reduce the size of our winner group by an additional 25% and we decided that this trade-off were too big.

Now we have got a definition of winners and losers. Winners are the schools that have 20% or more money after the reform and losers are the schools that got 20% less money. This is presented graphically in figure iv and as the data in table 4. In table 5 the distribution of winners and losers per district is shown. As we can see some districts had a lot of winners and some districts had none.

Figure iii: Half year cost per student

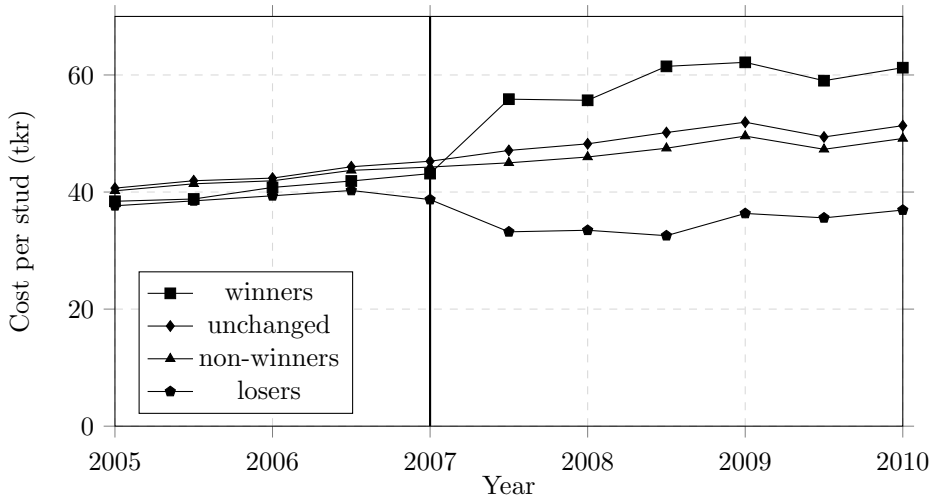


Figure iv: Inflation and time trend adjusted cost per student

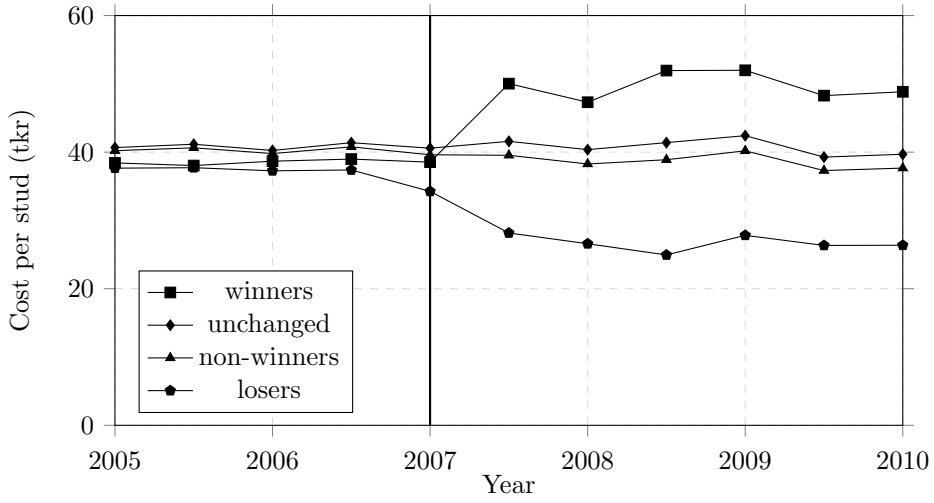


Table 4: Inflation and time trend adjusted cost per student

	Winners		Unchanged		Losers		Non-winners	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post
schools	8	8	39	39	7	7	47	47
mean	38 530	49 730	40 806	40 786	36 857	26 704	40 205	38 643
st.dev of mean	347	1 942	458	1 209	1 466	1 160	506	1 109
n	40	48	200	240	35	42	235	282

Note: The table is based on inflation and time trend adjusted cost per student for total 54 schools. The standard errors are calculated from the mean. All schools are observed 5 half years before the reform and 6 half years after.

Table 5: Winners and losers by district

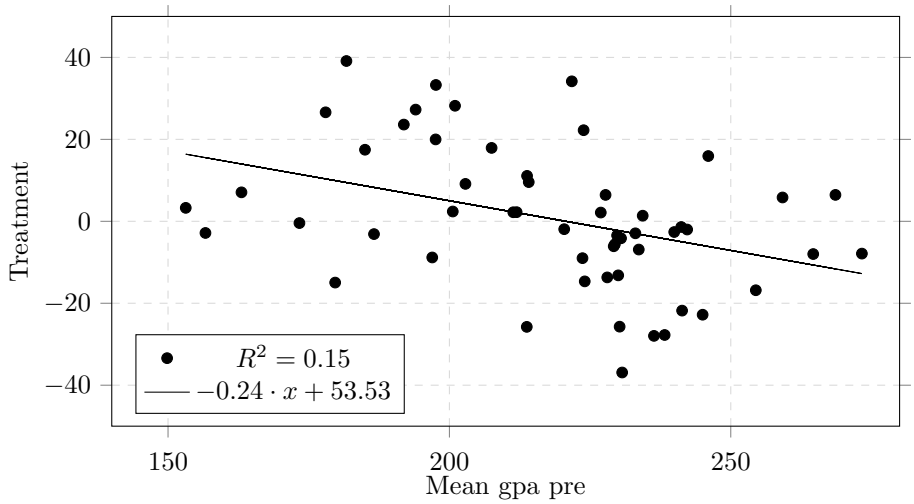
District	Unchanged	Winners	Losers	Total
Bromma	5	0	1	6
Enskeda-Årsta	3	0	0	3
Vantör	2	1	0	3
Farsta	2	1	0	3
Hägersten	2	0	0	2
Liljeholmen	2	0	0	2
Hässelby-Vällingby	5	0	0	5
Rinkeby	2	0	0	2
Kungsholmen	1	1	0	2
Norrmalm	3	0	1	4
Kista	2	0	0	2
Skarpnäck	2	0	0	2
Skärholmen	0	3	0	3
Spånga-Tensta	2	0	2	4
Maria-Gamla stan	3	0	2	5
Katarina-Sofia	2	0	0	2
Älvsjö	0	2	0	2
Östermalm	1	0	1	2
Total	39	8	7	54

Note: There is a map and a list in Appendix A that show where the districts are situated in Stockholm

We argue that the reform was exogenous to the school but it does not appear to be truly random which schools got more and which got less money. If it would have been, then the winners and non-winners should display the roughly the same socioeconomic composition and student performance as well as more even distribution between districts. Unfortunately this is not the case as can be seen in tables 6 and 5. It is however not very surprising we would expect some districts to be *fully*

treated. If also appears to be districts with lower socioeconomic status that are most treated, this is explained by the fact that the socioeconomic compensatory resources allocation now became mandatory. Far from all schools with low performing students was however treated. There are some, but rather low correlation between GPA pre-reform and the size of the treatment as figure v show. The random sampling is thus not perfect, but should not be a major threat to our project as long as there are no significant group specific changes to the socioeconomic composition at the winners and unchanged schools pre- to post-reform.

Figure v: Scatter plot with GPA pre-reform and treatment



To test this assumption we look at table 6 which shows the socioeconomic variables we observe reported as a mean before and after the reform. The plain difference imply that both foreign background and born abroad change differently between the two groups. To test the significance of these differences we run a Wilcoxon signed rank test (Cortinhas and Black 2012). The p-values from the test are reported in the table 6. With the exemption of foreign background in the non-winner group none of the changes in socioeconomic compositions are significant. From this we can draw the conclusion that the changes in socioeconomic composition of schools does not drive our treatment group classification, if that was the case we would have significant increases in the winner group. The sections above will be covered in greater detail in section 8.

Table 6: Socioeconomic composition pre- and post-reform

	Winners				
	n	Mean	Std. Dev.	Min	Max
born abroad pre	8	23	14.9	5.5	45
born abroad post	8	25	15.4	6	45.5
difference	8	2	4.8	-2.5	13
proc boys pre	8	54.3	5.6	45	62.5
proc boys post	8	52.1	5.6	44.5	62
difference	8	-2.2	4.9	-8	6
parent edu pre	8	2.1	0.2	1.9	2.4
parent edu post	8	2.1	0.2	1.9	2.4
difference	8	0.0	0.1	-0.1	0.1
foreign backgr pre	8	44.2	22.0	10.5	74.5
foreign backgr post	8	48.3	26.3	9.5	80.5
difference	8	4.1	7.9	-5.5	20
	Non-winners				
	n	Mean	Std. Dev.	Min	Max
born abroad pre	45	12.6	14.6	0	66.5
born abroad post	45	11.8	12.4	.5	57.5
difference	45	-0.8	5.2	-23.5	9
proc boys pre	45	50.7	7.0	27.5	61.5
proc boys post	45	51.8	6.6	35	66.5
difference	45	1.1	7.4	-20	17.5
parent edu pre	45	2.3	.3	1.6	2.7
parent edu post	45	2.3	0.3	1.6	2.8
difference	45	0.0	0.1	-0.2	0.2
foreign backgr pre	45	27.6	27.6	0	98.5
foreign backgr post	45	30.3	28.4	4	96.5
difference	45	2.8**	6.5	-6	23.5

Note: The differences are tested for statistical significance with a Wilcoxon signed rank test where the significance level are reported like this: *** p<0.01, ** p<0.05, * p<0.1

6 Empirical strategy

The leading method for analyzing panel data is to run an OLS regression on the variable of interest (Wooldridge 2013). That is also where we will start out. From the OLS estimation it is accustomed to either try a fixed effects model, which in our case would be to use school fixed effects, or a difference-in-difference approach. Wooldridge specifically points out that a DD-methodology is very useful when dealing with a quasi-experiment and that is why we will turn directly to a

DD-model after the OLS-estimation and not to a fixed effects model.

6.1 OLS

To estimate the effect of school funding on student performance we will start by making a simple OLS with the cost data. It will however most likely not give any reasonable results. First of all we know that our cost data are subject to some measurement error and it will most likely bias our results, more on this in the discussion. Furthermore we do not fulfill all the assumptions required to make an OLS BLUE, the first being random sampling. We can in hindsight see a clear pattern of who got more and less resources, this is shown in tables 5 and 6.

We can also assume that the OLS is subject to omitted variable bias; the main reason for not controlling for all known variables that could be correlated to our dependent variable is lack of data. We are thus violating the assumption of zero conditional mean (Wooldridge 2013). We will anyway estimate this model:

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 Q_{it} + \beta_3 \tau_t + \beta_4 \tau_t^2 + \epsilon_{it} \tag{2}$$

Where Y is average GPA, Pass or Grad at the school level i and academic year t (with $t = 06, 07, 08, 09$). X is the log of the inflation adjusted cost per student, this is our variable of interest. We use this rather than just cost per student in order to get the effect in percent change rather than in absolute change.[9] τ is a time trend (with $\tau = 1, 2, 3, 4$), τ^2 is the squared values of τ.[10] Q is a variable on the demographic composition of students. It includes parent education, parent abroad, born abroad and proc boys[11]. ϵ is the idiosyncratic error for each school and time period.

6.2 Difference in difference

As mentioned a more credible identification strategy for this quasi-experimental setting is a difference-in-difference model. To do this we need a treatment and a control group. Unfortunately no schools are untreated in this case. In fact all schools were affected by the reform, but with different magnitude. An ideal research design would be that only a few randomly selected schools would get more funding and the other would be totally unchanged. We try to imitate this scenario by using the winners, losers and unchanged categories defined in section 5. This is by default an arbitrary treatment and control group, but this method is no uncommon when there is a whole population that is treated to varying degrees (Fredriksson and Öckert 2007).

[9] This also requires the assumption that an increase in funding from 50 000 SEK per student to 60 000 SEK will give the same effect as an increase from 100 000 SEK to 120 000 SEK. We think this is a reasonable assumption. However if we do it with levels we get almost the exact same results.

[10] The reason being that grade inflation is higher for lower grades and then suffer from diminishing returns, this is due to grades not being able to go above 320

[11] See table 1 for full definitions

Before specifying the formal regression we are going to look at our variables for student performance to see if the common trends assumption that is necessary for a difference-in-difference estimation holds up. We want to see parallel trends in the outcome variable before the treatment. In figure vi, vii and viii we can analyze the trends in our measurements. We include both non-winners and the unchanged category. They are both plausible control groups. As the figures show they both indicate common trends for GPA and Grad. Pass however does not show any parallel trends for either group so we will not use that in our difference-in-difference regressions. The underlying values are also presented in table 7.

Figure vi: Average gpa

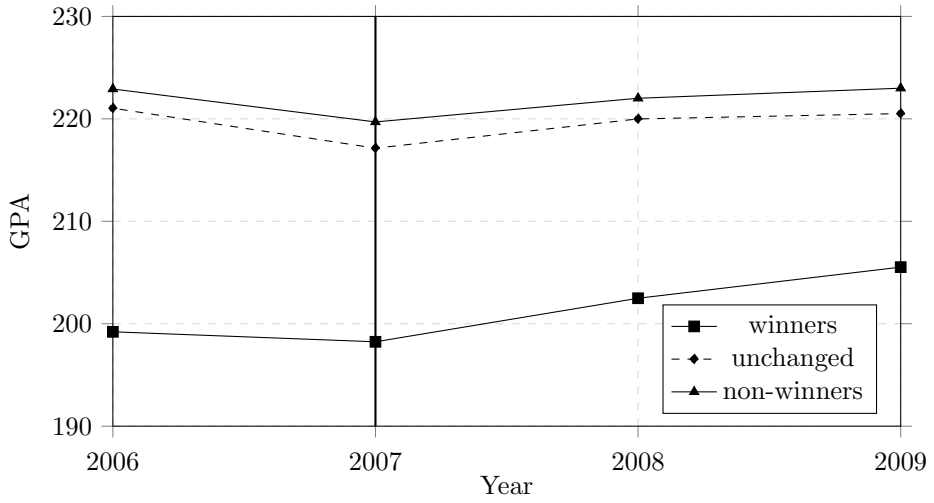


Figure vii: Average pass-rate

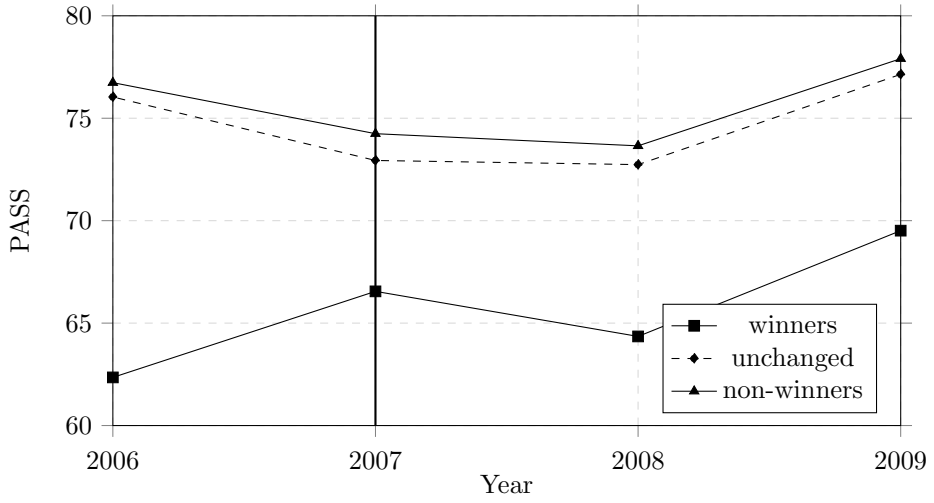


Figure viii: Average graduation rate

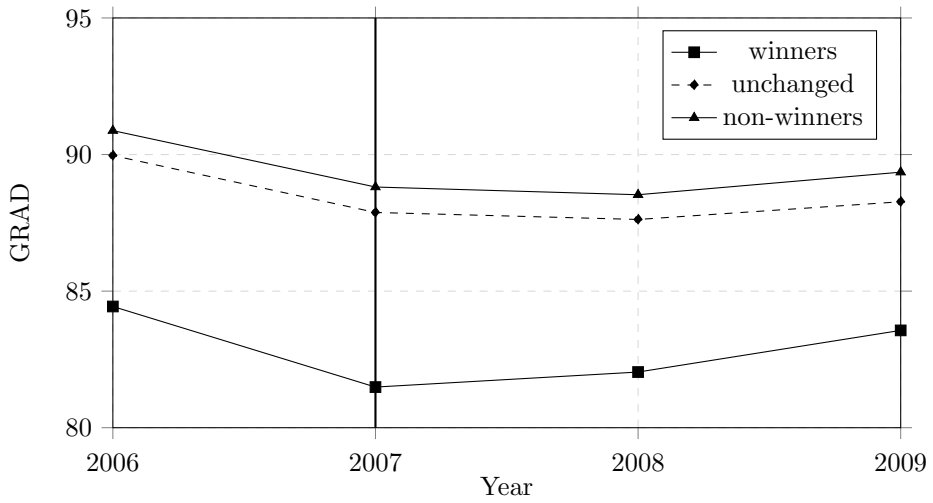


Table 7: Testing the common trends assumption

Variable	Obs	Mean	Std. Dev.	Min	Max
			Winners		
gpa pre	8	198.7	16.7	178	223.9
gpa post	8	204	20	181.4	232.4
grad pre	8	83	9.4	69	97.1
grad post	8	82.8	8.9	71.6	95.7
pass pre	8	64.4	13	46.2	79.5
pass post	8	66.9	11	54	84.5
			Unchanged		
gpa pre	39	219.1	29.5	153.1	273.3
gpa post	39	220.3	30.4	152.8	274.3
grad pre	39	88.9	12.3	51.7	99.7
grad post	39	87.9	12.9	47.8	100
pass pre	39	74.5	16.7	32	98.3
pass post	38	75.2	16.8	34.2	96.4
			Non-winners		
gpa pre	46	221.3	27.9	153.1	273.3
gpa post	46	222.5	28.8	152.8	274.3
grad pre	46	89.8	11.6	51.7	99.7
grad post	46	88.9	12.1	47.8	100
pass pre	46	75.5	15.6	32	98.3
pass post	45	76	15.5	34.2	96.4

Our first DD-model is the easiest two period difference-in-difference one can imagine:

$$Y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 Winner_i + \beta_3 (Post * Winner)_{ti} + \beta_4 Q_{ti} + \epsilon_{it} \quad (3)$$

Where Y is either average GPA or Grad at the school level i and academic year t (with $t = 06, 07, 08, 09$). $Winner$ is a binary variables that is 1 if a school is a winner and 0 if it is a non-winner as defined in section 5. $Post$ is a binary variables that is 1 if the academic year is 08 or 09 otherwise it is 0. Q are variables on socioeconomic composition[12] that are relevant to include. ϵ is the idiosyncratic error for each school and time period.

From this regression we will be able to derive how much GPA and Grad changed before and after the reform in the winners and non-winners groups. The interpretation of the coefficients will be as follows:

- GPA/Grad of non-winners pre-reform = $\beta_0 + \beta_4$
- GPA/Grad of winners pre-reform = $\beta_0 + \beta_2 + \beta_4$
- GPA/Grad of non-winners post-reform = $\beta_0 + \beta_1 + \beta_4$
- GPA/Grad of winners post-reform = $\beta_0 + \beta_1 + \beta_2 + \beta_3 + \beta_4$

Taking the GPA/Grad of the winners post-reform minus post-reform and then taking the difference from non-winners post-reform minus post-reform will give us the difference in difference estimator which as shown in equation 4 is = β_3.

$$((\beta_0 + \beta_1 + \beta_2 + \beta_3 + \beta_4) - (\beta_0 + \beta_2 + \beta_4)) - ((\beta_0 + \beta_1 + \beta_4) - (\beta_0 + \beta_4)) = \beta_3 \tag{4}$$

In addition to this model we will use a more sophisticated difference-in-difference model to try to capture the notion that the effect of increasing resources should be additive. There is a lot of support in the literature for what is called the added value hypothesis that the longer a student is exposed to more resources the larger the effect. In order for us to separate the effect from being treated one year or two years we will use this model that is common when analyzing a treatment that is thought to be increasing as time passes (Angrist and Pischke 2009).

$$Y_{it} = \beta_0 + \beta_1 Time_t + \beta_2 Winner_i + \beta_3 (Time * Winner)_{ti} + \beta_4 Q_{ti} + \epsilon_{it} \tag{5}$$

This is the same as in model 3 with the difference that $Time$ a vector of year dummies for 2006, 2007 and 2008 are included in the regression instead of $Post$. In contrary to the pooled post-reform period model this will allow us to look the effect of one year and two years of treatment separately.

With a difference-in-difference approach there is not always necessary to include background variables to control for composition effects if they are assumed to be a fixed effect. That is to say that there is no group specific time trends within the socioeconomic compositions of schools. To determine what background variables to add in our regressions we would like to know how they

[12] See table 1 for full definition

change pre- and post-reform and in the winners and non-winners group separately. To get this information we run a regression similar to model **??** but instead using each of the variables on socioeconomic composition, parent edu, born abroad, parent abroad and proc boys as Y.

$$Y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 Winner_i + \beta_3 (Post * Winner)_{ti} + \epsilon_{it} \tag{6}$$

Using the same interpretation as above we get some interesting information from this regression. If $\widehat{\beta}_1$ is significant then we know that there is a change in that variable pre- and post-reform for the whole population of schools. If $\widehat{\beta}_2$ is significant then we know that there is a significant difference between the winner and non-winners schools before the reform. And is $\widehat{\beta}_3$ is significant we know that there is a group specific change in that variables. Then it will be necessary to include it in our regressions.

6.3 Standard errors

The standard errors of a difference-if-difference estimation have been discussed by Bertrand et al (2004). who find that most studies using a difference-in-difference strategy understate their standard errors (Bertrand et al. 2004). This is because of serial correlation in the data. They argue that studies relying on longer time series are more vulnerable to this critique and since we only use four time periods this should not be a major problem. But we will also use two of the prescriptions Bertrand et al. suggest. The first is to ignore time series data which is what we do in 3. The second solution to the serial correlation problem is to cluster the standard errors. Angrist and Pischke argues that the standard errors will be underestimated if there are fewer than 42 clusters (Angrist and Pischke 2009). This criteria is for us fulfilled, since we have 54 clusters. We will thus use cluster robust standard errors at the school level in all our regressions and not just where a time series is included. We argue that these clusters are well-defined, which is a requirement for using cluster robust standard errors (Wooldridge 2013).

7 Results

7.1 OLS

Table 8 presents the results from the multiple regression. They all show a significant negative correlation between resources and students performance. There are strong reasons to believe that these results are biased, and this will be covered in more detail in the discussion.

Columns (1)-(3) shows that a one percent increase in cost would lead to a, -6.6, -3.8 and -2.8 change in GPA, Pass and Grad respectively. The constants in these regressions are the intercept of

the regression and does not tell us much since no school actually had zero funding. The coefficient for the constant however imply that a school that did not receive any funding would have a GPA of 962.2, which is impossible since there is a cap on GPA which is 320. The same goes for Grad and Pass which have a cap of 100.

Column (4)-(6) shows us the same regression but now with an added time trend and the results are almost the same as before. In column (7)-(9) we also add our background variables to account for composition effects. When including them we can see that we get coefficient closer to zero and also a higher R-squared. This would imply that we now explain more of the variation in our outcome variables and that correlation between the background variables and cost per student accounts for some of the negative effects on our outcome variables.

The R-square for the regressions in column (1)-(3) is as high as 0.453, this would imply that log cost explain roughly 40 percent of the variation in student performance. This is likely an overstatement that is caused by cost being correlated with other factors that have an impact on GPA. This is also what we would expect since the resource allocation model used in Stockholm was designed to compensate schools with weaker students. This would explain why we get so high values for R-square. Reversed causality might also be a problem here. We know that the resource allocation model does not take grades into account, but schools do received more funding after receiving more immigrant students, which in turn is associated with lower GPA, Grad and Pass.

To but the coefficient of interest from column (7) into a context they suggests that a 1 percent increase in costs per student would generate a 3.3 point decrease in GPA. If they were true then we would expect a decrease in GPA with over 60 points in the winner schools since all of them got more than 20 percent extra funding after the reform which is unlikely.

Table 8: Regression results from OLS

VARIABLES	(1) GPA	(2) Pass	(3) Grad	(4) GPA	(5) Pass	(6) Grad	(7) GPA	(8) Pass	(9) Grad
log cost	-65.54***	-37.56***	-28.41***	-67.85***	-38.87***	-28.96***	-32.91***	-19.55***	-16.00***
	(9.181)	(5.145)	(4.378)	(9.383)	(5.261)	(4.509)	(11.45)	(5.974)	(4.895)
parent edu							60.05***	12.65**	4.347
							(13.81)	(6.218)	(4.970)
parent abroad							0.212	-0.0965	0.0347
							(0.165)	(0.0865)	(0.0739)
born abroad							-0.0859	-0.301**	-0.397***
							(0.227)	(0.118)	(0.0894)
proc boys							-0.337**	-0.102	-0.0437
							(0.151)	(0.0873)	(0.0590)
time				-3.152	-5.619**	-3.711**	-5.288	-6.034**	-4.339***
				(3.086)	(2.309)	(1.616)	(3.312)	(2.327)	(1.489)
time2				1.315**	1.537***	0.898***	1.371**	1.488***	0.891***
				(0.579)	(0.434)	(0.318)	(0.632)	(0.449)	(0.295)
Constant	962.2***	500.0***	410.6***	986.4***	517.4***	419.5***	474.5***	282.4***	271.4***
	(103.6)	(57.68)	(48.94)	(105.9)	(58.56)	(50.53)	(148.1)	(75.05)	(61.50)
n	216	215	216	216	215	216	212	211	212
R-squared	0.427	0.438	0.453	0.447	0.467	0.464	0.672	0.669	0.660

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Clustered standard errors are not needed in the restricted model (column (1)-(3)) since there are no serial correlation when just doing a simple OLS. The standard errors reported are anyway clustered for consistency reasons. If only robust standard errors are used we still get the same level of significance. The standard error are actually lower with only robust standard errors.

7.2 Difference-in-Difference

When we run the difference-in-difference regressions we get a very different results compared to the standard OLS estimation. The results are presented in table 9. None of the difference-in-difference estimators are statistically significant but they all estimate a positive effect of being a winner. The coefficients on GPA range from approximately 1-3 points per year of treatment and for Grad the coefficient are less than 1 point.

Column (1)-(4) are the results from the two period difference-in-difference and column (5)-(8) are the results from the regression with year-dummies. In the two period regressions *Winner x Post* can be interpreted as the average effect of attending a winner school for 1.5 years. This is because both 2008 and 2009 is included in the regression and the average time in a winner school post-reform is then 1.5 years. *Post* indicate how much GPA and Grad changed for the non-winners. In (1) and (3) $Winners$ show that the winner schools had a significantly lower GPA and graduation rate before the reform. The constant is the GPA/Grad for the non-winners pre-reform. In (2) and (4) you need to add the coefficient of the background variables with the constant to get the true intercept. The coefficients on $Winner$ can in columns (2) and (4) be interpreted that there were no significant difference pre-reform in GPA and Grad once socioeconomic composition has been controlled for. In (2) and (4) *Winner x Post* increases for Grad and decreases for GPA in comparison to the regressions without background variables. Note also that *Post* becomes significant for Grad - meaning that there was a significant decrease in graduation rate after the reform across all schools once socioeconomic factors has been taken into account.

In column (5) and (7) *Winner x 2008* is the effect of one year at a winner school and *Winner x 2009* is the effect of two years at a winner school. As expected we can see an increasing effect as time passes from the reform. However in all regressions with year dummies the estimation of *Winner x 2008* and *Winner x 2009* are not statistically significantly different from each other. The year-dummies *2007, 2008 and 2009* show the *year fixed effect* that is to say the variation in GPA and Grad that can be attributed to a specific year. $Winner$ and $Constant$ is now the GPA/Grad for winners and non-winners 2006 rather than an average of 2006 and 2007 as it is in column (1)-(4).

Table 9: Regression results from DD on 20 percent

VARIABLES	(1) gpa	(2) gpa	(3) grad	(4) grad	(5) gpa	(6) gpa	(7) grad	(8) grad
Winner x Post	4.087	1.941	0.740	1.594				
	(5.088)	(4.145)	(2.476)	(2.074)				
Post	1.195	0.146	-0.902	-1.448**				
	(1.437)	(1.392)	(0.800)	(0.606)				
Winners x 2008					3.059	1.457	0.390	1.099
					(6.896)	(5.926)	(3.334)	(2.850)
Winners x 2009					5.114	2.507	1.089	2.161
					(4.623)	(4.190)	(2.432)	(2.191)
2007					-2.883	-2.288	-2.196***	-2.040***
					(1.832)	(2.004)	(0.803)	(0.743)
2008					-0.738	-2.324	-2.413**	-3.074***
					(1.884)	(2.107)	(1.091)	(0.936)
2009					0.244	0.248	-1.587	-1.926**
					(2.342)	(2.249)	(1.157)	(0.894)
parent abroad		0.196		0.0394		0.203		0.0445
		(0.181)		(0.0821)		(0.184)		(0.0828)
parent edu		71.01***		10.05*		71.46***		10.35*
		(13.62)		(5.553)		(13.60)		(5.488)
born abroad		-0.203		-0.485***		-0.202		-0.485***
		(0.259)		(0.0894)		(0.261)		(0.0889)
proc boys		-0.487**		-0.127		-0.473**		-0.116
		(0.203)		(0.0795)		(0.208)		(0.0816)
Winner	-22.58***	-6.253	-6.881*	0.395	-22.58***	-6.270	-6.881*	0.379
	(6.976)	(4.799)	(3.606)	(2.165)	(7.026)	(4.823)	(3.631)	(2.173)
Constant	221.3***	84.02**	89.84***	78.84***	222.7***	83.32**	90.94***	78.55***
	(4.133)	(33.72)	(1.715)	(14.22)	(3.974)	(33.78)	(1.667)	(14.10)
Observations	216	212	216	212	216	212	216	212
R-squared	0.067	0.608	0.038	0.568	0.069	0.610	0.043	0.574

Clustered robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: There are missing values in the background variables at one school that can make comparison between the regression with and without them less straight forward.

We included all the background variables in both regressions even though table 10 show no group specific time trends in any of them. However parent abroad changes significantly after the reform as the coefficient on *Post* indicate in column (2) table 10 and the winner schools had a significantly higher share of students that was born abroad and smaller share of students with highly educated parents before the reform as the coefficient on *Winners* show in column (1) and (4) table 10. There could possibly be some interaction effect between those factors that can influence GPA and Grad so we add all background variables in order to see how it changed our results. Interestingly the coefficient for GPA decreases and the coefficient for Grad did the opposite. It is a bit unclear how these results should be interpreted, but since adding background variables do not change the coefficients that much and since all variables of interest are insignificant it does not

seem to matter that much if they are included or not.

Table 10: Estimation on group specific trends in socioeconomic composition

	(1)	(2)	(3)	(4)
VARIABLES	born abroad	parent abroad	proc boys	parent edu
winners x post	2.756	-1.471	-3.288	0.0213
	(1.785)	(1.923)	(1.997)	(0.0312)
post	-0.756	3.533***	1.100	0.00867
	(0.776)	(0.802)	(1.111)	(0.00980)
winners	10.41*	6.261	3.590	-0.189**
	(5.461)	(4.650)	(2.165)	(0.0772)
Constant	12.59***	14.99***	50.72***	2.272***
	(2.181)	(2.280)	(1.056)	(0.0449)
n	212	212	212	212

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

We also included a correlation table and a VIF table in appendix C in order to control for multicollinearity in the regressions where the background variables are included. Some of the variables are highly correlated but does not appear to cause a multicollinearity problem.

For a bit more intuitive interpretation of the two period regression presented in column (1) in table 9 we also include table 11 where first difference, second difference and difference-in-difference are calculated along with the corresponding standard errors.

Table 11: Pooled difference-in-difference on GPA

	Winners	Non-winners	*Difference*
2006-07	198.7***	221.3***	22.6***
	(5.6)	(4.1)	(7.0)
2008-09	204.0***	222.5***	18.5**
	(6.7)	(4.3)	(8.0)
Difference	5.3	1.2	4.1
	(4.9)	(1.4)	(5.1)

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

To make sure that the results are robust, i.e. does not change dramatically when we use different cut-off points to define winners and losers we run the same regressions when defining winners with 25% and 15% as cut-off points. The estimations stay statistically insignificant but in the 25% group the coefficients are higher as expected but for some reason the effect is decreasing and not increasing from an extra year of treatment. In the 15% group the effect is almost zero for all

estimations with the exception of *Winner x 2009* for Grad which for some reason is higher than the 20% group. We also try excluding the loser schools from the control group so that we only compare the winners to the schools that were unchanged. Then the coefficients stay mostly the same. The results are presented in detail in appendix C.

8 Discussion

8.1 Aggregated data

One of the major problems with this study is that we do not observe our outcome variables; GPA, Pass and Grad, at an individual level, instead we compare school averages. This is also true for the background variables. In our regression we do not weight the observation in accordance with the number of students they were calculated from. A change in student performance and socioeconomic composition at a small or large school will thus artificially bias the results of any regression. This problem could probably be solved using a weighted least square method or by accessing individual data on the performance of all students. We can not do any of this within the scope of this thesis, but we can present the weighted average GPA by winners and non-winners. This is shown in table 3.1.

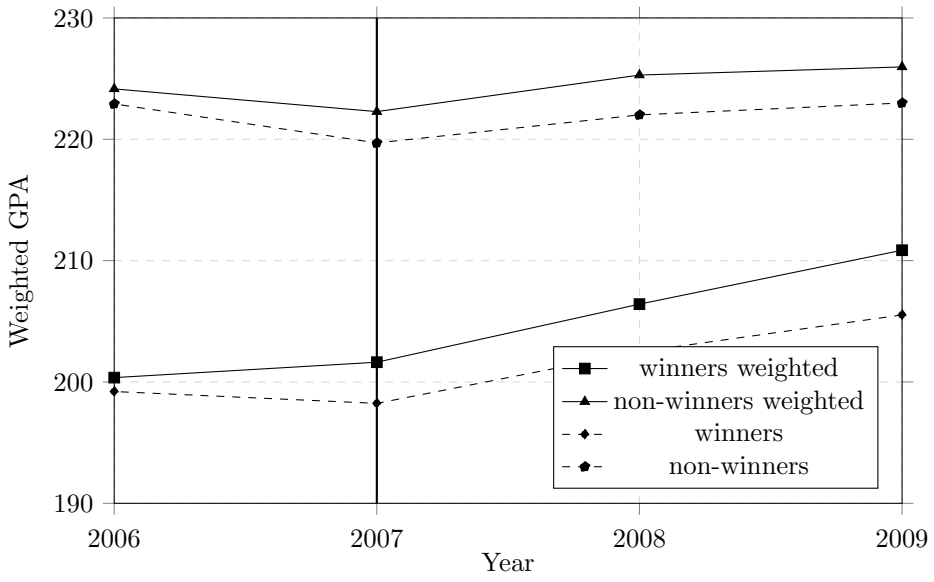
When using the weighted averages for a simplistic DD we also observe a higher difference-in-difference estimator than in the non-weighted data. The estimator rises from 4.1 (table 11) to 5.2 in table 12 . Without new data we are however not able to do a formal regression or calculate the standard errors. The results are also shown grafically with comparison to the non weighted data in figure ix.

Table 12: DD with weighted values

	Weighted GPA		
	Winners	Non-winners	*Difference*
2006-07	200.0	223.2	22.2
2008-09	208.6	225.6	17.0
Difference	7.6	2.4	5.2

Note: These values was created by calculating the share of graduating students each school had within its group (winners and non-winners) for every year that weight was then multiplied with the GPA of that school and year. The sum of those weighted GPA-values is what is presented in the table

Figure ix: GPA with weighted values



Note: This graph was created the same way as table 12. The unweighted trends are also included for easy comparison

Even though this is a major problem for this thesis it should be relatively easy to overcome if the individual data for the adequate years was collected. With individual data available more control variables could also be included in the regression and thus increase the precision. It would also be possible to do sub group analysis, for instance look specifically on student with foreign background or low socioeconomic status. The literature on school spending also suggest that the effect should be higher for those students.

8.2 Controlling for organizational effects

As mentioned in section 2 the change in resource allocation was not the only change that the reform in 2007 brought with it, there was also an organizational reform. One could assume that the management before the school had varying quality where some districts had more competent managers then others. If our winner schools, that were under the responsibility of only five separate districts before the reform, for some reason had relatively worse (or better) management than the non-winners that could bias our results.

It is possible in theory to test whether or not there was a group specific organizational effect for the winners. However our data does not allow for that and we have thus not included it in this thesis. The test would in short consists of creating a pseudo-treatment group with the schools that were in one of the five districts that had a winner school post-reform. Then the same DD-regression as we did in this thesis with the change that all the winner schools were dropped would be used to estimate the size of the organizational effect. The problem with our data is first and

foremost that all schools in districts 13 and 17[13] are winners so the organizational effect for those districts cannot be tested. And secondly there is no plausible common trend assumption between the pseudo-treatment group and the control group.

There might be other ways to control for this anticipated effect if better data or more sophisticated methods could be used.

8.3 Selection problem

When making a difference-in-difference regressions it is preferable if the sample or the treatment group was assigned using some kind of randomization, though this is not always possible, the reform we study being an example of this. The process was not completely random and the policymakers had a rough idea of how it would turn out for the different school. This causes a problem, since this will bias our data and it is hard to tell in which direction or how strong the bias will be. A question that arises is if we still measure resources effect on schools in general, or if we are actually measuring the effect on schools with a relatively high ratio of students with low grades and immigrant background?

There are a few problems associated with this, we will however just bring up two; the first being discussed up by David Figlo (1999). He argues that schools have different functional forms and that you cannot compare *rich schools* with *impoverished schools*. He further argues that in all the existing literature makes the assumption that the production function is homotheticity and additivity. What this implies is that we can expect the same output for different schools and different students. Figlo (1999) argues that one cannot expect an increase to have the same effect regardless of magnitude; student endowed effects, if the school is rich or if the school might just have a more innovative way of teaching. The relation is usually assumed to be the same, which Figlo argues is one reason for studies showing no results when studying resources and student performance (Figlo 1999).

If Figlo is correct, then it would be wrong for us to study *rich schools* with *impoverished schools*. This is however exactly what we are doing, or rather schools with high and low ratios of students with socioeconomic status. This would according to Figlo understate the effect of resources on student performance. This is problematic in many ways and even if we had had internal validity in this thesis there would be great threat to the external validity of the results.

We also argue that the overall trend in student performance is seemingly unchanged during these years. This would imply that there is something else driving the trend, which is in line with what we have seen in most of the literature. For instance a resent research review on what the driving forces behind the school results in Swedish primary school points at three dominant

[13] See appendix A to identify these

trends; segregation, decentralization and individualization. They argue that resources have some effect but more so for younger students and students with low socioeconomic status (Skolverket 2012). So even if we do have a problem with sample selection, we would argue that our results most likely would have come out the same, i.e. insignificant.

Using a suitable control group was another problem. In a perfect world we would like temporal stability, casual transience and unit homogeneity, though these assumption are not likely to ever hold in a school environment, since it is constantly exposed to different reforms. Furthermore we do have a problem with our control group being subject to the same reform as the treatment group. Though we were able to see from figure iv that the amount of resources stayed the same for some schools. We argue that this makes it possible to use them as our control group, since the reform barely affected those schools. In a perfect world we would like to study the counterfactual. That being the same group with and without treatment, though this is obviously impossible, hence the need to always compromise when it comes to treatment and control group. One alternative control group that could be explored in future research could be the independent schools in Stockholm who were left unchanged during this period.

Another note on the control and treatment group is that one would usually prefer to do a Wilcoxon signed rank test, in order to test for significant changes to the group composition. Though one assumption that needs to hold in order for this to work is that of the distribution of the variable being normally distributed or at least symmetrical. This assumption does however only apply if the amount of observations is smaller than 16. When we controlled for symmetry for our variables we found that none of them came close to being symmetrical. We are thus in violation of the assumption of normality in the treatment group, since this sample is smaller than 16. Our control group does however exceed 15 observations and is thus considered to be approximately normally distributed, despite having an underlying probability density function that is not. We did despite this try to make a Wilcoxon for both the winner and control group, even though we have a violation of the normality assumption in the winners group. The p-values did not come out as one would expect when comparing them to the changes between the groups and periods that we see when just eyeballing the absolute changes. Since the p-values in the winner group could and probably is biased due to the normality violation, we instead tried to measure the difference by making a difference-in-difference on the background variables in order to conclude which variables that might have changed significantly. We do this since we want to make sure that a school did not get increased funding due to getting a higher proportion of students that is associated with a higher grant, such as being born abroad.

This kind of reform could also create a self selection problem, that is to say that more student and in fact student with different characteristics would try to get into a school that got more

money. This would be a major problem if we had longer time series, but with our short time frame we do not think that a lot of students or parents would change school half the way through upper primary school[14].

8.4 Alternative types of measurement

Using GPA as a measurement for student performance over time has been criticized both for reliability and validity reasons. The literature on grading, fairness and their relationship to actual skills are vast and nothing we can comment on in the scope of this thesis. The reliability however is something that has been commented on from an economics perspective; especially the influence of grade inflation has been discussed (Skolverket 2009 IFAU 2011). It would thus be interesting to use some other outcome variable to see if the results stay the same. The preferred measurement is a standardized test as used by OECD in their PISA-study; this would more accurately measure actually knowledge rather than grades that could have been subject to grade inflation or unfair grading. The implication that this would have on our thesis is that we have not and were not able to control for actual knowledge, which would be preferable.

Another common measurement is life time earnings; this is used as a proxy for skills, which in turn is what the education system is used for, that being individuals acquiring skills. This is also data that we did not have when writing this thesis, though it would certainly be a better measurement than GPA.

8.5 Grade composition

The schools that we have in our data set consists of schools that have different grade compositions, ranging from having grade K-9 to just having 7-9. The reason why this might cause a problem is that we only know that the school got more money, though we do not know how, to which grade or on what the money was spent. What could have happened is that a school that we qualified as a winner might have spent all their extra resources on the students in grade 1-7 and leaving year 8-9 unchanged. If this would be the case, then the school should have been in the unchanged group instead of being in the winner group. This would lead us to observe the difference between students that actually got the same amount of resources both before and after the reform. We are however unable to control for this with the data available to us.

As we described in section 3 national tests in grade three were introduced in this period, this would influence the schools with grade 3 in their organization and not the others. It is not entirely unlikely that a school would steer additional money to lower ages if they know that those students will be tested for the first time.

[14]Grade 7-9

8.6 Omitted variable bias

When making our OLS-regressions we only controlled for a few of all possible OVB that there might be. This is due to limitations of our data set, a few examples of some variables that are often used when controlling for OVB that we did not have is: Peer effects, school fixed effects, teacher density, classroom size and teacher education, just to mention a few common ones. PISA made a comprehensive list of different control variables (OECD 2014) all of which we would ideally had used, though that would be beyond the scope of this thesis. This makes it hard for us to draw any inference from our OLS, since the assumption of zero conditional mean is violated. Especially when we know that we most likely have several biases that also work in different directions.

9 Conclusion

In this thesis we tried to answer the question: Will more funding generate better academic results in primary school? The data used when trying to answer this question was as mentioned in the data section collected from the following sources: The student performance, background and school size data comes from the two national databases SALSA (Skolverkets Arbetsverktyg för Lokala SambandsAnalyser), SIRIS (Skolverkets Internetbaserade Resultat- och kvalitetsInformationsSystem) and the data on school funding was collected from the School and Education Division and the District Councils in Stockholm City (Stockholm stad). The data was then used to make an OLS and a difference-in-difference estimation of the effects of increased funding, none of which produced any significant results.

The first conclusion of this thesis that we found is that the centralization reform in Stockholm created winners and losers in terms of funding. For most schools the resources did not change much, but on the extreme there were schools that got substantially more or less money. We also conclude that this reform should lend itself to interesting pseudo experimental studies on the effect of resources on student performance.

The second conclusion that can be drawn from this study is that our results indicate that there might be a positive effect on student performance of increasing school funding. However a significant correlation between resources and student performance cannot be established using our data and empirical method. The indications for an effect are both the contrast between the DD-estimations and the ordinary OLS-regression and the fact that longer time at a treated school renders a higher coefficient. We also see that a higher cut-off point in our definition of winners give higher coefficients and a lower cut-off point do the opposite. These indications are nevertheless not sufficient to draw any definite conclusions on the true relationship between resources and student performance.

The major problem with our data is the fact that we use aggregated data on student performance and not individual data. The observations on school level are thus not weighted for the number of students who graduate at a specific year. Our results could be biased for a whole host of different reasons but that is the most obvious flaw in our study. This was covered in more detail in section 8.

Had our results been significant and possible to draw trustworthy inference from then they would indicate an economically significant relationship between resources and student performance. The effect would be approximately 2.5 GPA points per year of treatment. The treatment being a 30 percent increase of total resources, which was the average increase in our winner group. If we assume that the effect is the same for every year with the treatment, which is a rather conservative assumption since some literature, suggest that resources have bigger impact on younger students (Lazear 2001), then we would multiply 2.5 with 9 to get the full treatment effect of increasing the school budget with 30 percent. That would analogously result in the rough estimation that a one percent increase in school budget would generate 0.75 points higher GPA and 0.25 percentage points higher graduation rate. This does however not take diminishing returns or possible nonlinear relationship between resources and grades into account.

If we compare these results to both the STAR-project and the results from Fredriksson and Öckert (2007), who found evidence for the same effect size as STAR we see that our effect size are roughly the same. In the STAR-project a class size reduction of seven students in four years (one year of preschool and grade 1-3) gave an effect of 0.2 standard deviations on test scores. To compare our results we need to translate a class size reduction to an increase in school funding. Krueger and Lindahl (2002) argue that the cost of a class size reduction is proportional to the percent increase in number of classes. Unfortunately there are no data on average class size in Sweden. Fredriksson and Öckert 2007 estimate the class size in 2002/03 to be 25 students per class. A reduction of seven student would then require somewhere around 30 percent increase in school funding. According to our estimations a 30 percent increase in resources for three years would generate an increase with 0.2 standard deviations for both GPA and Grad.

To put this in terms of policy, a one percent increase in primary school spending in Sweden would cost 0.88 billion SEK according to the Association of Regions and Local Authorities (Landsting 2015). Krueger and Lindahl 2002 use a projection of how increased test results impacts life time wages to calculate the long term return on investments from reducing class size with the same amount as the STAR-project and finds that this would be a economic sound investment.

We thus conclude that more research should be conducted on the subject and with the cost data that we have collected for this project. The data could not only be used to estimate the effect of increasing or decreasing school funding for an individual school. Since our accounting data are

quite rich we also assume that it will lend itself to study what schools that get extra money actually do with the new resources. Do they invest in more teachers? Smaller classes? Better teachers? The questions are many and are all very relevant for policymakers.

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Appendices

A School organization in Stockholm

Figure Aa: Map of Stockholm and the districts before the reform.

This is a map of Stockholm and the 18 districts that was responsible for primary schools before the reform.

Table A1: Districts

District	Number
Bromma	1
Enskeda-Årsta	2
Vantör	3
Farsta	4
Hägersten	5
Liljeholmen	6
Hässelby-Vällingby	7
Rinkeby	8
Kungsholmen	9
Norrmalm	10
Kista	11
Skarpnäck	12
Skärholmen	13
Spånga-Tensta	14
Maria-Gamla stan	15
Katarina-Sofia	16
Älvsjö	17
Östermalm	18

In our data all schools are given a number to indicate what district they belonged to before the reform.

B Data

Table B1: All school in our data-set

School-ID	Alt-ID	School name	Excluded	Dist.	Winner	Treatment
21724208		Sätraskolan	0	13	1	33.28
18023801	42149666	Husbyskolan/ Husbygårdsskolan	0	11	0	7.07
46581319		Lillholmsskolan	0	13	1	39.120596
59249600		Katarina Norra skola	0	16	0	-6.0894847
37416253	26852132	Spånga gymnasium, grundskolan	0	14	0	-25.72
60382920		Rågsvedsskolan	0	3	0	-2.84
87521048		Engelbrektsskolan	0	18	0	-22.80
24716970		Mariaskolan	0	15	0	-27.96
13679897		Bagarmossens skola	0	12	0	17.91

Continuation of Table

School-ID	Alt-ID	School	Outlier	District	Winner	Treatment
15594566		Äppelviksskolan	0	1	0	-16.82
51341279		Vinstagårdsskolan	0	7	0	2.23
82140090		Hässelby Villastads skola	0	7	0	-5.62
72672270		Aspuddens skola	0	6	0	9.57
16348406		Matteusskolan	0	10	0	15.93
99648792		Adolf Fredriks musikklasser	0	10	0	-7.89
17640364		Sundbyskolan	0	14	0	-25.76
36031993		Bredbyskolan	0	8	0	-3.12
75614609		Åsö grundskola	0	15	0	-21.80
69895255		Södermalmsskolan	0	15	0	-13.19
72248694		Enskede skola	0	2	0	2.13
43632318		Enbacksskolan	0	14	0	-14.95
72627865		Alviksskolan	0	1	0	-2.93
13124830		Sjöängsskolan	0	17	1	28.20
14121892		Bredängsskolan	0	13	1	27.26
43088454		Björkhagens skola	0	12	0	-13.69
41176931		Gärdesskolan	0	18	0	-1.39
19353396		Fruängens skola	0	5	0	17.47
97683561		Johan Skytteskolan	0	17	1	34.16
86597125		Hässelbyg.rdsskolan	0	7	0	19.99
18000402		Högalidsskolan	0	15	0	-6.89
27287797		Rinkebyskolan	0	8	0	-.43
88795871		Södra ängby skola F-9	0	1	0	-36.90
38758770		Kvickenstorpsskolan/ Farsta grundskola	0	4	1	26.60
34671128		Blommensbergsskolan	0	6	0	6.44
82110758		Hagsätraskolan	0	3	1	23.60
18029307	29410344	Stadshagsskolan/ Kungsholmens grund	0	9	1	22.23
61686233		Rålambshovsskolan	0	9	0	-3.45
50060022		Trollbodaskolan	0	7	0	-4.14
23617768		Sofia skola	0	16	0	-14.66
36281817		Höglandsskolan	0	1	0	6.45
83828066		Nya Elementar	0	1	0	-2.62
11218326		Ärvingeskolan	0	11	0	9.13
91816093		Sturebyskolan	0	2	0	1.37
52863414	71754599	Vasa Real	0	10	0	-27.74
57184925		Hökarängsskolan	0	4	0	-8.83

Continuation of Table

School-ID	Alt-ID	School	Outlier	District	Winner	Treatment
46165232		Mälarhöjdens skola	0	5	0	-7.99
69266159	18022102	Hjulstaskolan	0	14	0	3.28
48328583		Årstaskolan	0	2	0	2.39
44673074		Abrahamsbergsskolan	0	1	0	-2.00
94864296		Eriksdalsskolan	0	15	0	-8.00
20683111		Bäckahagens skola	0	3	0	2.21
93412255		Gubbängsskolan	0	4	0	-1.92
82850292		Vällingbyskolan	0	7	0	11.09
97686250		Rödabergsskolan	0	10	0	5.82
37796564	18032501	Akallaskolan/ Stenhagskolan	1	11	-	-
15331669		Solbergaskolan	1	17	-	-
89605548		Grimstaskolan	1	7	-	-
87838841		Smedshagsskolan	1	7	-	-
23618510		Nytorpsskolan/ Hammarbyskola	1	2	-	-

Table B2: Comments on data collection from the different districts

District	nr	Comments
Bromma	1	None
Enskede-Årsta	2	Month 13 was created with the available data and then added to the first halv year.
Vantör	3	None
Farsta	4	Even though half year cost are available we construct the half year cost variable since the wage cost is unevenly distributed over the years in the actual acounting. This is most likely an aconting error.
Hägersten	5	None
Liljeholmen	6	None
Hässelby-Vällingby	7	Do not have month 13 and it can not be created.
Rinkeby	8	None
Kungsholmen	9	None
Norrmalm	10	None
Kista	11	Accounting number 601 is excluded for rents instead of 510
Skarpnäck	12	None
Skärholmen	13	Accounting number 530 is excluded for rents instead of 510
Spånga-Tensta	14	Accounting number 610 is excluded for rents instead of 510. Actual half year cost was not available.
Maria-Gamla stan	15	Actual half year cost was not available.
Katarina-Sofia	16	Actual half year cost was not available.
Älvsjö	17	None
Östermalm	18	No detailed accounting available for 2005 so rent costs cannot be excluded, but the schools does not appear to be paying rent in 2006 and 2007 so it should not be a problem. Actual half year cost was not available.

Note: All monthly accounting is divided in 13 month where month 13 is a "correctional month" that is used for practical reasons. The cost attributed to that month, if any, should be included in the first half year to get a correct value. In the normal cases where month 13 was included in the records that is how we have done it.

Table B3: Comments on individual schools

School	Excluded	Dist.	Winner	Comments
Akallaskolan/ Stenhagskolan	1	11	-	In the last month of 2009 the cost for the caretaker is 10 million SEK instead of the usual cost at about 100 thousand SEK. This according to Stockholm City most likely an accounting error and thus the school is an outlier
Solbergaskolan	1	17	-	This school had a large division for student with intellectual disabilities and the accounting before the reform do not reflect the costs for that division. It is therefore impossible to compare the costs before and after the reform.
Grimstaskolan	1	7	-	In December 2008 the cost for inventories increases with 3 million SEK which cannot be explained
Smedshagsskolan	1	7	-	The cost for inventories increase the first half year from a usual 100 thousand SEK to 9 million SEK, this is probably an acounting error and the school will thus be excluded
Nytorpsskolan (Hammarbyskolan)	1	2	-	There is a irregular drop in the cost the second half year of 2007. The accounting reports a 10 million SEK lower total cost than the half year before and after
Ärvingeskolan	0	11	0	The increase in cost might adhere in majority to an increase in students with intellectual disabilities but it is hard to tell by the accounting.
Spånga gymnasium, grundskolan	0	14	0	This is a case where the national register and the municipality economic units does not match. When calculating cost per student students from Solhemsskolan are included.
Hjulstaskolan	0	14	0	This is a case where the national register and the municipality economic units does not match. When calculating cost per student students from Hyllingeskolan are included
Hökarängsskolan	0	4	0	Cost per student is not reliable because there is most likely another school unit included in the accounting. It stayes the same pre- and post-reform however.
Stadshagsskolan/ Kungsholmens grund	0	9	1	This is a case where the national register and the municipality economic units does not match. When calculating cost per student students from Fridhemsskolan 1-5 are included.
Husbyskolan/ Husbygårdsskolan	0	11	0	This is a case where the national register and the municipality economic units does not match. When calculating cost per student students from Husbygårdsskolan K-5 are included, the schools however merged totally 2009 so after that the cost per student can be calculated using the standard procedure.
Vasa Real	0	10	0	The accounting seem to include the school Gustav Vasa so their students are included to calculate cost per student.
Eriksdalsskolan	0	15	0	Here the time series are calculated on only the 7-9 students from 2007:2-2009:2. The explanation is that the 7-9 part of the school was its own economic unit for 2.5 years after the reform.
Bredbyskolan	0	8	0	Values on Pass are missing because it is calculated on fewer than 10 individuals. Then the value is not reported in the national databases out or integrity reasons.
Högalidsskolan	0	15	0	Values on socioeconomic composition are missing for all time periods because they are not reported in the national database. There is no explanation available for this.

Figure Ba: Average inflation and time trend adjusted cost per student (cut-off 15 percent)

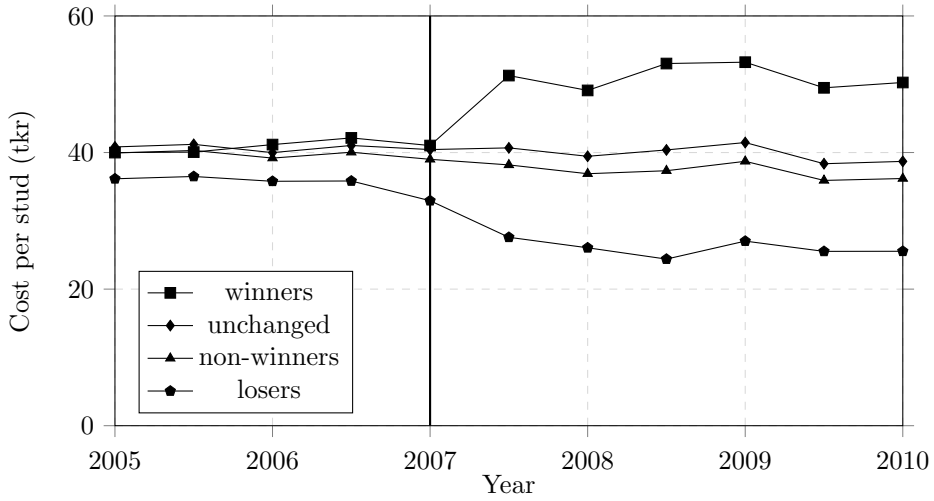
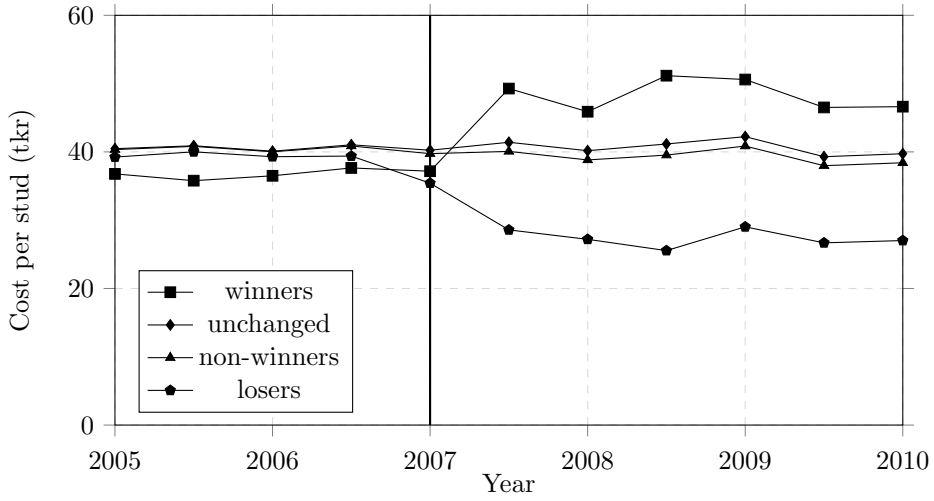


Figure Bb: Average inflation and time trend adjusted cost per student (cut-off 25 percent)



C Results

When making our regressions we wanted to make sure that we controlled for collinearity, in order to do this we constructed a correlation matrix, see tabel **??**. The correlation for most of our variables is fairly high, so we calculated the VIF for the most specified OLS- and DD-model in order to make sure that the variables do not inflate our standard errors. There is no clear rule on an exact value for VIF that you can use in order to check if it is ok to use the variable or not, but a general rule of thumb is that a VIF below 10 is OK (Cortinhas and Black 2012). The VIFs for our variables are all under 10 and we are thus able to use all of them in our regressions, even though the correlation between them is fairly high.

Table C1: Cross-correlations

VARIABLES	gpa	grad	pass	log cost	parent abroad	parent edu	born abroad	proc boys	winners	treatment
gpa	1									
grad	0.88	1								
pass	0.90	0.91	1							
log cost	-0.65	-0.67	-0.66	1						
parent abroad	-0.55	-0.52	-0.61	0.46	1					
parent edu	0.75	0.65	0.72	-0.56	-0.80	1				
born abroad	-0.63	-0.73	-0.70	0.53	0.64	-0.76	1.			
proc boys	-0.21	-0.16	-0.16	0.19	-0.00	-0.08	0.09	1		
winners	-0.26	-0.19	-0.22	0.11	0.12	-0.21	0.29	0.09	1	
treatment	-0.37	-0.29	-0.33	0.29	0.13	-0.26	0.29	0.26	0.70	1

Correlation on data from 2006-2009
Observations: 211

Table C2: VIF table for OLS

VARIABLES	VIF
parent abroad	8.20
born abroad	5.11
parent edu	4.12
log cost	1.56
proc boys	1.05

Table C3: VIF table for DD

VARIABLES	VIF
parent abroad	2.98
born abroad	2.56
parent edu	4.03
proc boys	1.05
winners	2.10
winners x 2008	1.72
winners x 2009	1.71
2007	1.51
2008	1.77
2009	1.76

Table C4: Regression results from DD on 25 percent

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	gpa	gpa	grad	grad	gpa	gpa	grad	grad
Winneri x Post	6.403	2.535	2.617	2.212				
	(6.018)	(5.249)	(2.850)	(2.640)				
Post	1.089	0.150	-1.083	-1.449**				
	(1.406)	(1.341)	(0.780)	(0.586)				
Winner x 2008					8.665	4.813	4.002	3.280
					(7.961)	(7.359)	(3.395)	(3.325)
Winner x 2009					4.142	0.291	1.231	1.186
					(4.848)	(4.640)	(3.079)	(2.725)
2007					-2.883	-2.272	-2.196***	-2.034***
					(1.832)	(2.005)	(0.803)	(0.742)
2008					-1.248	-2.634	-2.800**	-3.261***
					(1.874)	(2.049)	(1.110)	(0.913)
2009					0.542	0.609	-1.563	-1.718*
					(2.344)	(2.206)	(1.126)	(0.870)
parent abroad		0.196		0.0373		0.201		0.0416
		(0.180)		(0.0827)		(0.183)		(0.0835)
parent edu		71.11***		9.988*		71.52***		10.27*
		(13.75)		(5.515)		(13.71)		(5.447)
born abroad		-0.211		-0.479***		-0.208		-0.478***
		(0.256)		(0.0918)		(0.259)		(0.0915)
proc boys		-0.488**		-0.126		-0.480**		-0.118
		(0.204)		(0.0788)		(0.210)		(0.0816)
Winneri	-25.07***	-5.437	-9.608**	-0.486	-25.07***	-5.451	-9.608**	-0.497
	(7.153)	(5.435)	(3.684)	(2.366)	(7.204)	(5.461)	(3.710)	(2.372)
Constant	220.7***	83.63**	89.89***	78.98***	222.2***	83.31**	90.99***	78.90***
	(4.009)	(33.96)	(1.654)	(14.11)	(3.840)	(33.98)	(1.594)	(13.99)
Observations	216	212	216	212	216	212	216	212
R-squared	0.060	0.606	0.050	0.568	0.062	0.608	0.055	0.573

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table C5: Regression results from DD on 15 percent

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	gpa	gpa	grad	grad	gpa	gpa	grad	grad
Winner x Post	0.0214	-0.377	0.655	1.845				
	(4.487)	(3.542)	(2.337)	(1.866)				
Post	1.795	0.648	-0.938	-1.584***				
	(1.386)	(1.444)	(0.753)	(0.574)				
Winner x 2008					-0.624	-2.069	0.0792	0.631
					(5.481)	(5.298)	(2.853)	(2.645)
Winner x 2009					0.667	1.324	1.230	3.071
					(4.959)	(3.303)	(2.603)	(1.899)
2007					-2.883	-2.316	-2.196***	-2.035***
					(1.832)	(1.991)	(0.803)	(0.743)
2008					-0.146	-1.523	-2.373**	-3.001***
					(1.932)	(2.162)	(1.099)	(0.954)
2009					0.854	0.441	-1.699	-2.251**
					(2.265)	(2.360)	(1.120)	(0.880)
parent abroad		0.159		0.0288		0.165		0.0335
		(0.182)		(0.0835)		(0.185)		(0.0839)
parent edu		68.38***		9.513*		68.78***		9.768*
		(13.57)		(5.665)		(13.55)		(5.602)
born abroad		-0.210		-0.473***		-0.210		-0.473***
		(0.261)		(0.0944)		(0.263)		(0.0939)
proc boys		-0.438**		-0.119		-0.428**		-0.112
		(0.185)		(0.0723)		(0.191)		(0.0759)
Winner	-20.32***	-8.671*	-5.972*	-1.864	-20.32***	-8.671*	-5.972*	-1.867
	(7.061)	(4.759)	(3.126)	(2.345)	(7.112)	(4.798)	(3.148)	(2.361)
Constant	222.5***	89.10**	90.15***	80.13***	223.9***	88.76**	91.25***	80.12***
	(4.346)	(33.65)	(1.834)	(14.52)	(4.185)	(33.71)	(1.781)	(14.42)
Observations	216	212	216	212	216	212	216	212
R-squared	0.089	0.619	0.040	0.569	0.090	0.621	0.045	0.575

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table C6: Regression results from DD on 20 percent with alternative control group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	gpa	gpa	grad	grad	gpa	gpa	grad	grad
Winner x Post	4.116	1.992	0.812	1.648				
	(5.156)	(4.120)	(2.524)	(2.104)				
Post	1.165	0.00819	-0.974	-1.492**				
	(1.623)	(1.376)	(0.924)	(0.675)				
Winner x 2008					2.857	1.149	0.374	0.963
					(6.954)	(5.911)	(3.383)	(2.846)
Winner x 2009					5.374	2.930	1.250	2.414
					(4.759)	(4.284)	(2.509)	(2.289)
2007					-3.404	-2.292	-2.232**	-2.000**
					(2.041)	(2.299)	(0.922)	(0.856)
2008					-0.797	-2.112	-2.415*	-2.958***
					(2.065)	(2.154)	(1.259)	(1.068)
2009					-0.276	-0.226	-1.766	-2.082**
					(2.685)	(2.420)	(1.348)	(1.020)
parent abroad		0.144		0.00780		0.148		0.0117
		(0.158)		(0.0797)		(0.161)		(0.0802)
parent edu		75.54***		12.32**		75.83***		12.53**
		(13.50)		(5.531)		(13.49)		(5.478)
born abroad		-0.239		-0.494***		-0.238		-0.495***
		(0.231)		(0.0817)		(0.233)		(0.0812)
proc boys		-0.547**		-0.130		-0.531**		-0.118
		(0.236)		(0.0965)		(0.244)		(0.0999)
Winner	-20.36***	-3.649	-5.959	1.632	-20.36***	-3.676	-5.959	1.611
	(7.371)	(4.342)	(3.749)	(1.902)	(7.432)	(4.366)	(3.780)	(1.912)
Constant	219.1***	77.13**	88.92***	73.95***	220.8***	76.73**	90.04***	73.78***
	(4.753)	(33.25)	(1.987)	(14.34)	(4.559)	(33.39)	(1.931)	(14.28)
Observations	188	184	188	184	188	184	188	184
R-squared	0.056	0.673	0.029	0.647	0.058	0.674	0.034	0.651

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1